# A STUDY IN TONAL ANALYSIS. I.

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The experiments on which the present study is based are directed toward the general problem of the psychophysical analysis of tonal stimuli, a problem which finds its natural setting in the conflict of current theories of audition. The primary function of a psychophysical theory of audition is the explanation of analysis. Except under unusual conditions, the stimulus presented to the ear is complex; but, if the stimulus is periodic, it is, as a rule, broken up by the auditory apparatus into simpler components, each of which gives rise to a single simple sensation of tone. The first task of auditory theory lies, therefore, in the establishment of the physical and physiological conditions underlying the production of these simple tonal sensations.

The traditional theory of Helmholtz considers the ear as a resonance mechanism, which analyzes complex periodic disturbances in the sense of Ohm's law. That is, the theory regards the auditory apparatus as a mathematical analyzer of great capacity and precision. The resonance theory, now almost a half century old, was a stroke of genius. By the application of a comparatively simple physical principle it rationalized a large and tangled mass of facts. It is not, however, without its weak points. The number of facts which any adequate theory must take into account has increased enormously in recent years, and, in some respects, the facts may be said to have outgrown the theory as Helmholtz propounded it. The result is a bewildering number of new theories. Some of these are modifications of the traditional theory; they are built upon Helmholtz's foundation.[1] Others reject the principle of sympathetic analysis by tuned fibres, and offer in its

[1] *E. g.*, H. Ebbinghaus, *Psychologie*, I, 313 ff. L. Hermann, Zur Theorie der Combinationstöne, *Arch. f. ges. Phys.*, XLIX, 499 ff.; Beiträge zur Lehre von der Klangwahrnehmung, *Ibid.*, LVI, 467 ff. H. Ayers, Vertebrate Cephalogenesis. II. A Contribution to the Morphology of the Vertebrate Ear, with a Reconsideration of its Functions, *J. of Morph.*, VI, 1 ff. C. Stumpf, *Tonpsychologie*, II, 480 ff.

place some other means of analysis.[1] The general objection to Helmholtz is that he demands too much of the auditory mechanism. Thus Ebbinghaus maintains that the vibrating fibres are less elastic, and therefore that sympathetic resonance is more susceptible of disturbance than Helmholtz had supposed. The more freely and independently the fibres respond, he argues, the less clearly and completely will they analyze the initial stimulus. Again, Max Meyer objects that our knowledge of physical objects does not warrant the assumption that bodies so minute as the basilar fibres can vibrate sympathetically to tones lying near the lower limit of audition. The usual argument that the difference between the high and low fibres is to be explained by difference of load, is not justified, he thinks, by any known physiological facts, though the loading required for this purpose would be very great. Meyer holds, moreover, that the Helmholtzian theory is incapable of explaining certain important phenomena brought to light by recent studies of combination tones.

The general facts of auditory analysis are patent and all current theories attempt to meet them; but the degree and the facility of analysis of which the auditory apparatus is capable are still matters of dispute, and it is clear that no definitive theory of auditory sensations can be formulated until the limits and accuracy of tonal analysis are better known than they are at present. In view of this fact, it occurred to the writers that a study of analysis under somewhat unusual conditions might throw light on this most fundamental problem of auditory theory. The unusual conditions were to consist in using as a stimulus a simple periodic vibration of constant rate but of regular and rapid changes of amplitude. It seemed probable to the experimenters that the ear might be expected to solve this rather difficult problem differently according as it proceeded by resonance or by some less delicate and less accurate means of analysis.

## Preliminary Experiments.

Since our stimulus was to be as simple and controllable as possible, we decided against Kœnig's siren disc method, which

[1] *E. g.*, Max Meyer, Über Kombinationstöne und einige hierzu in Beziehung stehende akustische Erscheinungen, *Zeit. f. Psych. u. Phys. d. Sin.*, XI, 177 ff.; Zur Theorie der Differenztöne und der Gehörsempfindungen überhaupt, *Ibid.*, XVI, 1 ff.; Über die Intensität der Einzeltöne zusammengesetzter Klange, *Ibid.*, XVII, 1 ff. Emile ter Kuile, Die Übertragung der Energie von der Grundmembran auf die Haarzellen, *Arch. f. ges. Phys.*, LXXIX, 146 ff.; Die richtige Bewegungsform der *membrana basilaris*, *Ibid.*, 484 ff. J. R. Ewald, Zur Physiologie des Labyrinths. VI. Mitth. Eine neue Hörtheorie, *Arch. f. ges. Phys.*, LXXVI, 147 ff.

compounds a series of shocks or puffs grouped in periods of variable intensity.[1] For a similar reason, we soon abandoned the interruption method of varying, by means of a rotating disc, the intensity of a continuously sounding tuning fork.[2] We regarded this method as particularly objectionable for preliminary work, inasmuch as it involves the interruption or interference tones.[3] The tuning fork without the disc promised a simpler and more easily controlled source of sound. The tuning fork is, however, extremely rigid, and does not, therefore, easily and quickly yield to differences of strain from external force.

Two methods were used to overcome this difficulty and to induce rapid and periodic oscillations of intensity from the sounding fork. In the first method, the fork employed as stimulus was immersed in heavy oil and provided with a small temporary magnet placed between the ends of the prongs. The magnet was put in the circuit of an actuating fork whose period was one-half or one-third the period of the stimulus-fork. The stimulus-fork was thus driven (by arranging the contact-time of the primary fork) every second or third vibration, and immediately damped by the heavy oil in which it was immersed. The fork was provided with a short writing point and recorded its movements immediately upon a kymographic drum. It was found impossible, however, to damp the stimulus-fork, by this means, in the short intervals between the successive contacts of the slower fork. Further to offset the high elasticity of the stimulus-fork, a second circuit, with a second primary fork, was introduced in such a way that one magnet set between the prongs, and a pair of magnets set opposite the outer faces of the fork, tended to produce alternate rest and motion of the prongs (second method). High stress within the fork again defeated our aim, the fork writing a compound curve which represented the superposition of the rates of the two primary forks upon the proper rate of the stimulus fork itself. In both of these experiments the cores of our magnets reproduced very clearly the tone of the actuating fork, thus complicating the stimulus.

This direct means of producing periodic changes of amplitude was now abandoned and a third method introduced. In the new method, a tuning fork was revolved at a constant rate about the longitudinal axis of its stem.

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[1] R. Kœnig: *Quelques expériences d'acoustique* (1882), 131 ff.

[2] *Cf.* K. L. Schaefer and O. Abraham, Studien über Unterbrechungstöne, *Arch. f. ges. Phys.*, LXXXIII, 207 ff.; LXXXV, 536 ff.; LXXXVIII, 475 ff.; R. Kœnig, *op. cit.*, 138 ff.

[3] The term 'interruption tone' seems to come from Stefan. See Über einen akustischen Versuch, *Sitzungsber. d. kais. Akad. d. Wiss. zu Wien* (Math.-naturwiss. Cl.), 1866, LIII, Abt. 2, 703.

## The Literature of the Rotating Fork.

Experiments with rotating forks are by no means new. As early as 1825, E. H. and W. Weber described in their *Wellenlehre* such an experiment, as follows: "If a tuning fork is put into a lathe so that it can be rotated about the longitudinal axis of its stem, it is found that the fork ceases to sound when a certain rate of rotation is reached, but that the tone reappears if the lathe is suddenly stopped. This is not to be explained by supposing that the noise of the lathe drowned the fork, for if one brings the end of a cylindrical tube close to the prongs of the fork and puts the other end to the ear, one is convinced that the rotation does not destroy the vibration of the fork, but prevents its transmission to the air. We can give no explanation of this remarkable phenomenon."[1]

W. Beetz next took up the work and repeated the experiment of the Webers.[2] He did not find, however, that the tone of the fork disappeared, but only that its intensity was diminished. He heard also a higher tone and a series of puffs, equal in number to double the number of rotations of the fork. He was not able to explain the phenomenon. Later Beetz again took up the experiment,[3] using two forks of 512 and 1024 vibrations per second. When these forks were rotated about twelve times per second, Beetz found that the pitch of the lower fork was raised about three-fourths of a tone, and the higher about a half tone. He heard again the beats, two for every revolution of the fork. The phenomenon, he holds, is not to be connected with the transmission of the vibrations of the fork to the air, for one hears the rise in pitch just as well, or better, when one lays one's head on the lathe and stops one's ears entirely. Beetz attempts to explain the rise in pitch by the supposition that the fork here becomes a special case of the Foucault pendulum. In a later investigation[4] he explains that he has discovered a source of error in his experiments which renders this explanation impossible. He also points out the existence of certain lower tones, apparently subjective, which would also be inexplicable by the theory of the Foucault pendulum. Accordingly, after repeating all his experiments, Beetz attempts to get an explanation from Doppler's law in regard to changes of pitch with a moving source of sound. The tonal qualities present with the revolving fork could very well be explained in this way, but Beetz was entirely unable to make out any quantitative cor-

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[1] Quoted by W. Beetz in an article Über die Töne rotirender Stimmgabeln, *Poggendorff's Annalen*, 1866, CXXVIII, 490 ff., and referred by him to the *Wellenlehre*, 510. The same reference is given by Stefan, Nachtrag zu dem Aufsatze: Über einen akustischen Versuch, *loc. cit.*, LIV, Abt. 2, 600. Exner and Pollak (Beitrag zur Resonanztheorie der Tonempfindungen, *Zeit. f. Psych. u. Phys. d. Sin.*, XXXII, 310) also quote the passage and refer it to the *Wellenlehre*, 110. Since the Webers' book is not at our disposal, we cannot tell which reference is correct.

[2] This was described in a paper before the Physical Society at Berlin, July 4, 1851; mentioned in *Die Fortschritte der Physik*, VI u. VII, 1850-51, viii.

[3] Über die Töne rotirender Stimmgabeln, *Poggendorff's Annalen*, 1866, CXXVIII, 490 ff. This was translated into English with a note by G. C. Foster, *The Philosophical Magazine and Journal of Science*, 1866, Series 4, XXXII, 534 ff.

[4] Über die Töne rotirender Stimmgabeln. Zweite Notiz. *Poggendorff's Annalen*, 1867, CXXX, 313 ff.

respondence. The observed intervals were much too large for those computed by Doppler's formula.

While Beetz was performing these experiments, J. Stefan also was carrying on investigations of a similar nature.[1] He found that, if a vibrating plate were rotated before the ear,[2] the characteristic tone of the plate disappeared and was replaced by two tones, the one higher, the other lower, than the primary. The higher is usually the stronger of the two, and the primary tone is sometimes audible along with the lower and higher tones. The same phenomenon is heard with a rotating tuning fork. The phenomenon to be explained, according to Stefan, is the effect upon the ear of a tone of periodically varying intensity. The movement which a tone of constant intensity produces in a body vibrating in sympathy with it can be expressed in the formula

$$a \sin 2 \pi n (t+\theta)$$

where $n$ = vibration rate, $t$ a variable and $\theta$ a constant time, and $a$ the amplitude of vibration. If the intensity varies periodically, $a$ becomes a periodic function of $t$ and in the simplest case can be expressed as

$$\alpha \sin 2 \pi n' (t+\theta');$$

$n'$ being the number of intensity changes in a unit of time. $\alpha$ is then a constant quantity. If, now, one substitutes this formula for $a$ in the first, one gets for the excursion of the sympathetically vibrating body

$$\alpha \sin 2 \pi n' (t+\theta') \sin 2 \pi n (t+\theta)$$

or

$$\frac{\alpha}{2} \cos 2 \pi (n-n') (t+\theta_1) - \frac{\alpha}{2} \cos 2 \pi (n+n') (t+\theta_2).$$

But each of these expressions represents a simple pendular vibration, the one having a vibration rate of $n-n'$, and the other a rate of $n+n'$.[3] By actual observation, Stefan found that his lower and higher tones corresponded in pitch to the demands of this explanation.

In a second article,[4] Stefan describes some other experiments in which he produced periodic changes of intensity by rotating a perforated disc before a sounding fork, and gives credit to those who had preceded him in the work with rotating plates and forks. Meanwhile Radau,[5] without performing any experiments, had anticipated Stefan's calculation and reached the conclusion that a tone varying periodi-

[1] Über einen akustischen Versuch, *Sitzungsber. d. kais Akad. d. Wiss. zu Wien* (Math.-naturwiss. Cl.), LIII, Abt. 2, 696 ff.

[2] Stefan also rotated a sector over the plate at rest, getting the same result.

[3] *Cf.* Rayleigh's analysis of the wave obtained by interrupting the tone of a fork with a perforated rotating disc. He finds it to be composed of three simple vibrations, having the frequencies $n$, $n+m$, and $n-m$, where $n$ = the rate of the fork and $m$ the number of intermittences. Acoustical Observations, III, *The Philosophical Magazine and Journal of Science*, 1880, Series 5, IX, 278 ff. *Cf.*, also, *The Theory of Sound*, 2d ed., I, 1894, 71 ff.

[4] Nachtrag zu dem Aufsatze: Über einen akustischen Versuch, *Sitzungsber. d. kais Akad. d. Wiss. zu Wien* (Math.-naturwiss. Cl.), LIV, Abt. 2, 597 ff.

[5] *Moniteur scientifique*, 1865, 430. So Stefan (*Ibid.*, 598) gives the reference; Exner and Pollak (*loc. cit.*, 312) refer it to the same year, 136. We have not had access to a file of the periodical.

cally in amplitude should produce the two tones which Stefan actually found. Radau coined the name 'variation tones' for the phenomenon. Stefan recognizes also the priority of the work of the Webers and of Beetz.

Upon the appearance of Stefan's articles, Beetz again took up the experiments with a view to testing the hypothesis of Radau and Stefan.[1] Finding the hypothesis well borne out with rotating plates, he turned again to rotating forks to determine whether they too gave the tones to be expected from Stefan's formula. He found that the lower tone as observed corresponded approximately to the calculated tone. The higher tone, however, was always much higher than the theory required. The difference between the observed and calculated values became very large with rapid rates of rotation. Beetz found, however, that the difference became trifling when he took his observations with a resonator having an opening 5mm. instead of 25mm. in diameter. With such a resonator the observed values coincided very closely with the values computed by Stefan's formula. Beetz used three forks, $c_1 = 256$, $a_1 = 440$, $c_2, = 512$, and three rates, 6.5, 13, and 19.5 revolutions per second. He took, also, some observations for two very low forks, 64 and 77 vibrations. In almost every case, Beetz's observed values are larger than the calculated values. In this last paper, Beetz accepts Stefan's and Radau's explanation of the phenomenon.

All the experiments described above were undertaken by physicists in the interest of a physical theory of sound. The most recent investigation with the rotating fork is that by Exner and Pollak,[2] of the Physiological Institute of the University of Vienna, who use it in the interest of a psychophysical theory. They propose to test the resonator theory of audition by using simple tones with periodic reversal of phase. They reason as follows. When a wave train acts on a properly tuned resonator, the effect, up to a certain limit, is cumulative; *i. e.*, each successive wave increases the sympathetic vibration of the resonator until the limit is reached. If, however, the wave suddenly changes phase, its energy will be directed against the inertia of the resonator, and the two will oppose one another until equilibrium is reached, after which the wave will again produce on the resonator its former cumulative effect. If, now, this change of phase is made periodically, it should result in a wave with much smaller amplitude than the original wave, periodically varying in intensity, unless the phase changes follow one another so closely that the wave is entirely annihilated. Hence it should follow that, if audition is mediated by a series of resonators, a tone thus interrupted should be discontinuous and we should hear bursts of sound alternating with periods of silence. It should follow further that, by keeping the intensity of the tone constant and increasing the frequency of the phase changes, we can cause the tone to decrease in intensity until it entirely disappears. That is, the cumulative effect on the basilar resonators of the waves falling between any two successive phase changes will not be sufficient to raise the nervous impulse above the limen of sensibility. If, now, the number of phase changes is kept constant and the physical intensity of the tone is increased, the tone which has become just inaudible should be lifted over the limen.

Exner and Pollak used three forms of experiment to obtain the conditions which they required: (1) a tuning fork rotated about the

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[1] Über den Einfluss der Bewegung der Tonquelle auf die Tonhöhe, *Poggendorff's Annalen*, 1867, CXXX, 587 ff.

[2] Beitrag zur Resonanztheorie der Tonempfindungen, *Zeit. f. Psych. u. Phys. d. Sin.*, XXXII, 305 ff.

longitudinal axis of its stem and having, therefore, four phase changes for each revolution; (2) a stationary fork which actuated a telephonic diaphram under a current which was periodically reversed by means of a rotating commutator, thus causing two changes of phase at each rotation; (3) a rotating stop-cock which brought alternately to the ear the waves from the side and from the face of a continuously sounding fork. The results reached by these methods confirm the authors' hypothesis regarding auditory resonance. They found that the sudden reversal of phase, when it comes with sufficient frequency, destroys the tone. A critical rate of phase-change was discovered. At this rate (which was fairly constant under the given conditions) the sound of the tuning fork disappeared, and reappeared only when the rate of revolution was diminished.

It will readily be seen, now, that the purpose of the Viennese investigators coincided approximately with ours; it was, namely, to place the observer under a set of conditions so unusual and at the same time so well controlled that the result should serve as an *experimentum crucis* of the theory in question. Notwithstanding this general coincidence, we shall proceed at some length with the discussion of our own experiments, both because we have laid greater emphasis upon the introspective record and because we have considered additional points of method and of interpretation.[1] The two sets of results present, as we shall see, important and fundamental differences. Before passing to our own experiments, we should repeat, for the sake of clearness, that the theoretical conclusions which Exner and Pollak draw are based wholly upon the sudden changes of phase which the tonal stimulus, as produced by the rotating fork, undergoes; we have found it necessary, however, to consider not only phase changes but also the periodic variations of amplitude, which seem to us to be of no less importance in the psychophysical interpretation of the acts.

## New Experiments with the Rotating Fork.

*Apparatus and Method.* In most of the observations recorded below, a Kœnig fork of 384 double vibrations ($Sol_3$) was used; a Kœnig fork of 128 ($Ut_2$) was employed, however, in a few minor determinations (see p. 493). The higher fork was set horizontally[2] in close-fitting, Y-shaped bearings, the stem of the fork being inserted in a hollow steel shaft, 11 cm. long and 12 mm. in diameter. The hollow end of the shaft was split across one diameter and the fork-stem was held in place by means of a collar and set-screw. The end of the shaft opposite the fork was provided with a compound pulley wheel of three grooved discs whose diameters approximated respectively 2.5, 5, and

[1] The problem was already well under way when the report from the Vienna Laboratory came to our notice.

[2] The lower fork because of its great weight was set upright.

7 cm. The base of the Y-shaped bearing was screwed to a heavy block of wood, which, in turn, was secured to the surface of a solid bench. The fork and shaft of the rotator were driven by a belt-gear apparatus with six wheels. The highest and lowest rates required more gear-wheels and a cone-reducer. The whole apparatus was turned by hand. Its rate was kept constant by turning in unison with the sound of a metronome conveyed from an adjoining room through a tin speaking tube, which terminated in two rubber tubes for the ears of the experimenter. The sound of the metronome was not audible to the observer. The rate of the rotating fork was registered by means of a speed indicator set directly into the end of the shaft opposite the fork. By varying the metronome rates and shifting the gearing, the writers were able to produce with great constancy any rate of revolution between the limits of ten and eighteen hundred in the minute (.6 and 30 in the second). Both investigators served alternately as experimenter and observer, each rate being both produced and read off at least once by each of them. No observations were made where the two readings on the indicator varied more than ten revolutions in the minute. For the slower rates the range of error was five revolutions or less.[1]

When once the rate had been determined, the fork was struck with a felt hammer, damped near the base to eliminate overtones, and set into rotation by one of the writers. As soon as the swing of the metronome had been caught, a signal was given to the observer who brought a Kœnig resonator near the side of the revolving fork and adjusted the length of the resonator until the resonance was maximal. After this a full introspective account of the tone was given. In case two or more tones were present, the point of maximal resonance for each tone was determined. Full observations were taken by both of the investigators at each rate. It was found that after some preliminary practice the place of maximal resonance could be determined with considerable accuracy. Since, however, the permanent calibration on the resonators was found to be unreliable, the resonators were standardized with a set of Kœnig forks (the same 'maximal resonance' method being used) and the intermediate spaces on the resonator were corrected by reference to the new standards. The two observers agreed well in the choice of points of maximal resonance.

[1] This mode of rotating the fork was adopted after various electrical devices had been tried and rejected. The writers were, at first, confident of improving on Alfred M. Mayer's primitive hand-rotator for driving disc sirens; but they learned, in the course of their experiments, to appreciate the wisdom and judgment of this clever and accurate acoustician.

Where the final settings of the resonator differed slightly, the mean of the two settings was taken as the basis for calculation. It was, of course, inevitable that the two observers should sometimes differ, for maximal resonance is an area, not a line. This means of determining the vibration rate of a given tone involves a certain range of error, due (1) to the magnitude of the limen for intensive differences; (2) to the comparatively wide selectiveness of the resonator; and (3) to the unjust divisions of the areas between the standardized values (*e. g.*, between maximal resonance points for $Sol_3$ and $La_3$, $Sol_3$ and $Fa_3$, etc.).

In regard to the defects of the resonator method, we may say that, in the first place, it was the best that our materials offered, and was used while we were waiting for more adequate means of control with which to continue our study. In the second place, the method gave results; and, in the third place, it was possible to check it from time to time, and thus to standardize the actual error involved.[1]

INTROSPECTIVE RESULTS AND THEIR INTERPRETATION.

I. The lowest rates of rotation gave precisely what one hears by revolving a sounding fork slowly before the ear: four bursts of tone separated by intervals of weak sound.[2] Our fork gave a more intensive tone from the faces than from its narrow edges and, as a result, the four bursts of sound were alternately strong and weak, giving in all eight intensities (2 maximal, 4 minimal, and 2 medium) at each revolution. At a moderate rate (about 80 revolutions in the minute) the minimal intensities disappear, leaving two maximal and two medium or moderate intensities. Thirdly, at about 225 revolutions the separate pulses of sound disappear, leaving a rough, noisy, throbbing complex, which resolves itself into a higher tone (A) and a lower tone (C). With a higher rate, there appears between these two tones a complex of tone and noise which, with a further separation of A and C, becomes the tone B. The pitch of tones A and C diverges more and more, as the rate of rotation is increased.

II. In counting the beats produced by B and a second, stationary fork of 384 vibrations, it was found that the actual drop of B was less than the drop observed with the resonator. This may be accounted for by the indefinite quality of B and the consequent difficulty of obtaining exact maximal resonance,

[1] The writers hope to be able to present in a second paper full numerical results obtained under an adequate method of control.

[2] *Cf.* H. von Helmholtz, *Sensations of Tone*, Trans. by Ellis, ed. 1895, 161.

and also, perhaps, by an error of expectation, which may have been influential after a decline in pitch was once noted.

III. The results show a conspicuous parallelism between the rise and fall of tones A and C, and between these and the gradual acceleration of the rate of rotation. The drop in C seems to be uniformly more rapid than the rise in A and, moreover, the rise in A is always less than, and the drop of C always greater than, the rise and fall computed from Stefan's formula. These discrepancies might well be supposed to be connected with the fall in the pitch of B. But the drop of B comes much later and is also much less in amount than the deviations of A. and C.

IV. On discovering that an evident relation obtained between tone B and the proper rate of the fork, we conjectured that the gradual drop in pitch of this tone was due to a change in internal strain of the fork and that this, in turn, was to be ascribed to the action of centrifugal force. To test this conjecture, we substituted magnetic for centrifugal force. A pair of electromagnets were placed close to the outside faces of the fork. We first used a Kœnig fork of 128 vibrations and determined its rate by counting beats with a fork of 128+,—first, when the current was passing through the magnets and, secondly, with an open circuit. Though our results with this method seemed to show a drop in the rate of the fork when the current was on, we did not regard them as entirely satisfactory. The tone of the fork was extremely weak when the magnets were in circuit and died out very rapidly. The longest period over which we were able to count beats was, therefore, only five seconds. To verify our results we used a 50-fork with the graphic method, controlled by a Jacquet chronoscope ticking fifths of a second. Two records without the current gave the rates 48.75 and 49.64 vibrations per second, and one record with the current a rate of 45.40 vibrations. Since, however, the record showed a large variation in the number of vibrations from time-unit to time-unit, we suspected that the Jacquet control was not accurate. Accordingly, we substituted a Kronecker interrupter, vibrating 20 times per second, and went back to our 128 fork. By counting full seconds on the drum record, we were able to get fairly accurate accounts of the vibration-rate. The result was as follows:

Current off (average of three records) $127.55 \pm .35$

Current on (average of two records) $126.42 \pm .22$

From these results it appears that, under the conditions named, a force acting perpendicularly to the faces of a tuning fork will reduce its vibration-rate. We inferred from this fact that the drop in the pitch of tone B was due to centrifugal force acting on the rotating fork. It also becomes evident, from our

experience with forks vibrating in a magnetic field, that outward strain, due to magnetic or centrifugal force (as the case may be), is responsible, at least in part, for the rapid damping of the fork and the consequent early disappearance of the tone produced. It is certainly true that the intensity of the tone dropped at once as soon as the fork was set in rotation.

V. The beats given by tones A or C with standard forks of approximately the same pitch were of a peculiar character. They were not regular beats such as one usually hears with forks of nearly equal pitch. The beats were quite clear and distinct and there can be no doubt that they were present only when the second fork was sounding. The time intervals between the successive bursts of sound, however, were not equal. The most graphic way of expressing their character would be to say that the beats 'dribbled.' Moreover, there was no observable sequence which repeated itself, so far as we could discover. The beats came in no temporal pattern or rhythm, but seemed to be quite arbitrary in their arrangement. Nevertheless, it was sometimes possible to count them and, strangely enough, the count appears to show that the number of beats is indicative of the pitch of the tone. No causal relation that we could discover obtains between these beats and temporary variations in the rate of rotation. The beats from tone B were entirely normal.

## Criticism of Exner and Pollak.

In the light of our own experiments, we may venture to make the following criticisms of the earlier experiments of Exner and Pollak. Though these writers regard the rotating fork experiments as crude, and base their conclusions on the telephone and rotating cock experiments, they get essentially the same results by each of the three methods they employ. It would seem, therefore, that our results offer a sufficient basis for a criticism of their conclusions.

(1) The conditions of the experiment were unfavorable for observation. The mode of procedure consisted in starting from a pure tone and gradually increasing the number of phase changes until the observer judged that the tone had disappeared. The experimenters admit that this was a most difficult task, especially in view of the fact that the tone never actually did become inaudible. They refer the continuous sound to the fact that the fork set in vibration the objects to which it was attached. They assert, however, that good results were obtained when the observer limited his attention strictly to the discontinuous sound which was conveyed to him through the ear-tubes.

(2) There was probably a good deal of noise in the course

of the experiment. The fork was electrically driven and was rotated by an electric motor. In the last two forms of experiment the commutator and the rotating stop-cock probably made some noise and the motor was in use in both these latter experiments. The observer was in a room apart from the apparatus and was forced to take indiscriminately all that came to him through the tube. In our own work, where we used a comparatively noiseless apparatus and had it constantly under the observer's eye, we found that a little practice easily enabled us to identify and to abstract from the extraneous noises of the apparatus.

(3) Though Exner and Pollak make a good deal of the importance of using pure tones in their investigation, it is almost impossible that their telephone experiment could have fulfilled this condition. The tones of vibrating plates are notoriously complex, and it is very improbable that the investigators were able to obtain a telephone diaphram which reproduced accurately the pure tone of their fork. They have nothing to say about a test of the telephone in this respect.

(4) Exner and Pollak pay curiously little attention to the pitch of the tone from the rotating fork. Since they were interested only in the point at which the tone disappeared, they assumed that the division of the tone which all experimenters after the Webers noticed was of no importance for them. It certainly is not obvious that this is the case. Moreover, they believe that in the last two forms of their experiment they have succeeded in eliminating this division of the tone. This they assume because in these cases they did not observe any change of pitch. Considering the general disregard of pitch which the rest of the experiments show, we are inclined to think that this is not conclusive against the presence of the phenomenon. Moreover, it is not clear how it was possible to eliminate the division by the means used, unless Stefan is entirely wrong in ascribing it to change of intensity. The latter must certainly have been present. In the last series of experiments, the revolving cock certainly did not cut off the sound instantaneously and must, as it opened and closed, have produced changes of intensity. In fact, it appears that Exner and Pollak have here essentially the conditions which Schaefer and Abraham used to produce their 'interruption tones.'[1] But if we grant that the reversal of the current in the telephone circuit produced a movement of the diaphram approximating Exner's ideal curve, though this seems extremely unlikely, the assumption of a sympathetically vibrating membrane in the

[1] Studien über Unterbrechungstöne, *Arch. f. ges. Phys.*, LXXXVIII, 475 ff.

ear would certainly imply differences of intensity arising from such a mechanism. The whole point of their argument rests on the fact that successive waves act cumulatively upon the resonating fibres and that a change of phase means the introduction of an oppositely directed force and a consequent reduction of the amplitude to zero. The curves which Exner and Pollak reproduce in their article show precisely those changes of amplitude which Stefan presupposes in his theory. Of course, we realize that Exner and Pollak are in no way committed to Stefan's explanation; but they have nothing to say against it, and it is not clear whether they mean to reject it off-hand or whether they believe that they have really produced changes of phase without changes of intensity.

In one respect our results agree perfectly with those of Exner and Pollak. The intensity of the tone decreases very noticeably when the fork is set in rotation. This decrease Exner and Pollak ascribe entirely to the phase changes; as we have seen, it is at least partly due to the centrifugal force acting upon the rotating fork. If the fork rotates slowly, the tone comes in puffs as described above (p. 492). This beating tone, according to Exner and Pollak, gradually lost its tonal character and became a whirring noise. Only two of their observers who were musicians noted a rise in the pitch of the tone. According to our observations, the sound of the moving fork was a very rough tonal complex, which, at the lower rates of rotation, our introspections describe as 'throbbing' or 'beating.' The separate tones of this complex, when brought out clearly with the resonator, were by no means smooth, the lower appearing rougher than the higher. The complex was clearly tonal in its character, however, and showed no tendency to become a 'whirring noise.' In fact, the separate tones became smoother as the rate increased and we are inclined to think that this was the case also with the complex as a whole. We do not believe, either, that the throbbing and beating character of this tone is to be ascribed to the presence of the phase changes. At the low rates of revolution the tones A and C would lie so close together that they would beat or produce a very inharmonious complex. Thus, for example, Helmholtz got beats between two interruption tones where the source of sound was a single fork.[1]

The assumption of Exner and Pollak that the intensity of the tone would approach the limen as the rapidity of rotation increased was verified in their experimental results. For us this result did not appear at all. A rate of three thousand revolutions in the minute left the tones as distinct and, to all

[1] *Sensations of Tone*, Trans. by Ellis, ed. 1895, p. 420.

appearances, as intensive as at the lower rates of rotation.[1] There was a very decided drop in intensity when the fork was set in rotation, but after the fork was once well started there was no appreciable decrease with any rate which we used.

Exner and Pollak found that the tone of their 240 fork disappeared when the fork was rotated six times in the second. There would here be twenty-four phase changes in the second and ten vibrations of the fork between each two successive phase changes. Hence they argue that, for a tone of the intensity they used, ten vibrations are necessary in order to stimulate the nerve endings above the limen of sensibility. The results of all their experiments are essentially the same, though the number of vibrations between phase changes at the point of disappearance differs slightly. Here again it will be seen that our results are entirely at variance with those of Exner and Pollak. Our rate of thirty revolutions in the second gave only 3.2 waves between successive phase changes and our highest rate (fifty per second) gave but 1.92. Yet, as we have said, the clang of the fork was clearly audible and not noticeably diminished in intensity. It is but fair to say, however, that Exner and Pollak make no claim of universality for their figures in this case. The number of waves necessary to excite an audible tone depends entirely on the amplitude of the waves. It is quite possible, therefore, that even less than a single wave might produce an audible tone. The discrepancy is so marked, however, that it seems worth pointing out, for there is no reason to suppose that the tone from our fork differed greatly in intensity from that of the fork used by Exner and Pollak.

In general, our results bear out the experimental data of Stefan. The rotating fork produces not its pure fundamental, but a tonal complex which can be analyzed not into Stefan's two tones, but into *three* tones, one lying very near to the fundamental but falling gradually in pitch as the rate of rotation increases, and two others lying about equidistant above and below the fundamental. The latter tones separate farther and farther as the rate of rotation rises. Whether the pitch of these two tones corresponds exactly to the pitch computed by Stefan's formula, we shall consider in our second paper. We may say, however, by way of anticipation, that there seems to be a constant discrepancy, which suggests the possibility that Stefan's explanation may stand in need of correction. For the theory of Exner and Pollak,—that rapid alternations of phase

[1] This was the highest rate which our apparatus permitted us to reach. We could not maintain it sufficiently long or with sufficient accuracy to get detailed results.

reduce the intensity of the tone below the limen of audibility,—we find no evidence in our experimental results. The complex from the rotating fork retains its tonal character and persists,—after a certain minimal rate is reached,—with undiminished intensity up to the highest rates of rotation which we have been able to employ. We have found, however, in the strain of rotation an alternative explanation for the lesser intensity of the moving fork.

*(To be concluded.)*